

Appl. No. 10/009,910  
Reply Dated May 17, 2005  
Reply to Examiner's Answer of March 24, 2005



Attorney Docket No. 81839.0107  
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IN THE UNITED STATES PATENT AND TRADEMARK OFFICE

In re application of:  
Makoto IIDA, et al.  
Serial No: 10/009,910  
Confirmation No.: 7347  
Filed: December 12, 2001  
For: SILICON WAFER, SILICON  
EPITAXIAL WAFER, ANNEAL  
WAFER AND METHOD FOR  
PRODUCING THEM

Art Unit: 1722

Examiner: Matthew J. Song

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John P. Scherlach 05/17/05

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Dear Sir:

Please find enclosed an original plus 3 copies of the Reply Brief.

If for any reason a fee is required, please charge any insufficiency to Deposit Account No. 50-1314. A copy of this Letter is enclosed.

Respectfully submitted,

HOGAN & HARTSON L.L.P.

Date: May 17, 2005

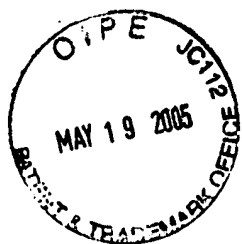
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PATENT  
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Name

*John P. Scherlecher* 05/17/05

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Date

**REPLY BRIEF**

Mail Stop Appeal Brief – Patents  
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Alexandria, VA 22313-1450

Dear Sir:

Applicants have received an Examiner's Answer with a mailing date thereon of March 24, 2005. Pages 13-18 thereof contain a section entitled "(10) Response to Argument". Such section sets forth certain new arguments. Applicants are filing this Reply Brief in order to address such arguments.

Page 13 discusses two outstanding issues, the first of which is a 112, first paragraph, new matter rejection and the second of which is whether the invention would have been obvious to a person of ordinary skill in the art at the time of the invention. In relation to the first such issue, the Examiner states on page 14 that the original disclosure does not teach a correlation between the number of oxygen

precipitation nuclei and the number of BMDs. It is taught that BMD density is strongly dependent on initial oxygen concentration, and therefore because other factors may contribute to the final density BMD, there is no direct correlation between oxygen precipitation nuclei and BMD density.

In response, Applicants point out that as discussed in the specification, the oxygen precipitation nuclei can be grown to be BMD by heating treatment, and therefore there is indeed direct correlation between oxygen precipitation nuclei and BMD density (see lines 16-23 of page 9, the last line of page 9 through line 4 of page 10, lines 3-8 of page 16, line 21 of page 17 through line 2 of page 18, lines 10-17 of page 23, line 25 of page 5 through line 10 of page 6, and so forth). It is natural that the initial oxygen concentration is strongly dependent on BMD density. Many oxygen precipitation nuclei are generated if the initial oxygen concentration is high, and consequently many BMDs grown from them are generated. The Examiner's Answer makes the fallacious argument that because other factors may contribute to the final density BMD, there is no direct correlation between oxygen precipitation nuclei and BMD density.

Applicants further traverse the argument set forth at line 18 of page 14 through line 3 of page 15 of the Examiner's Answer. It is argued that the instant specification does not teach this feature. However, it is understandable that such an impossible theory isn't described in the original specification. What is described in the specification and what should be sufficient is that precipitation nuclei grow to be BMDs by heating treatment.

The Argument at line 16 of page 15 through line 7 of page 16 of the Examiner's Answer is unclear and evidences a misunderstanding of the technical issue. As previously described by Applicants, distribution of the crystal defect in the silicon single crystal is determined by the parameter of  $V/G$ , which is the ratio of the pulling rate  $V$  and the temperature gradient  $G$  (see page 4, the last paragraph in the specification of the present Application, lines 50-60 of column 1 of Iida et al., and the claims in Hourai et al.). Also, the pulling rate  $V$  can be controlled to change freely in the range, for example, of 0.4-1.0 mm/min as described

in Iida et al. However, the temperature gradient  $G$ , which depends on the hot zone structure of the CZ furnace, is determined by the structure of the furnace used. Therefore, for changing  $G$ , it is necessary to change the internal structure of the furnace before growing crystal, and it is almost impossible to control change in the temperature gradient  $G$  during the growing of the crystal (see column 9, line 41 through column 10, line 15 of Iida et al., lines 59 through the end of column 1, and lines 44-58 of column 4 of Hourai et al.). Accordingly, the experimentation as presented by Iida et al. that the pulling rate  $V$  is controlled to change into, for example, 0.4-1.0 mm/min and that the distribution of the crystal defect is observed by splitting the grown crystal longitudinally may be carried out at only one time because the same internal structures of the furnace have same temperature gradient  $G$ . Because the same internal structure of the furnace can fix the temperature gradient  $G$ , the distribution of the crystal defect is determined only by the pulling rate  $V$ . Conversely, with no change of temperature gradient  $G$ , the distribution of the defect is determined only by the pulling rate  $V$ , and therefore only the same result can be obtained by the additional experimentation to change the pulling rate, as in Iida et al.

Here, the temperature gradient doesn't change even though the crystal is doped with very little carbon in pulling the crystal. Accordingly, in doping the crystal with carbon, one of ordinary skill in the art would have no idea how to carry out the experimentation of changing the pulling rate  $V$  as in Iida et al. regardless of the absence of any change in the temperature gradient  $G$ .

Doping the crystal with carbon does not change the temperature gradient  $G$ , and therefore the  $V/G$  value can be controlled only by the pulling rate  $V$ . Therefore, one of ordinary skill in the art would naturally think that the crystal having the desired defect distribution (here, the N-region) can be obtained by pulling the crystal by the pulling rate  $V$  as known in the art.

However, the present invention demonstrates that the pulling rate  $V$  for the crystal to be the N-region can be improved faster than in the case of the prior art by carbon-doping with no change in the temperature gradient  $G$ . Because the pulling

rate  $V$  for the N-region is changed regardless of the absence of any change of the temperature gradient  $G$ , the  $V/G$  value for the N-region shifts with carbon-doping. The present invention is the first time that such a dopant-dependence of the  $V/G$  value has been recognized. Therefore, it would not have been obvious to one of ordinary skill in the art. Preferably, the present invention defies the conventional wisdom of one of ordinary skill in the art that the distribution of the crystal defect is determined by the pulling rate  $V$  with no change in the temperature gradient  $G$ , because the distribution of the crystal defect is determined by the  $V/G$  value as described above. Also, the crystal has an N-region of the same value of the pulling rate  $V$  with the prior art if the temperature gradient  $G$  does not change (the distribution of the crystal defect is determined by the  $V/G$  value). Consequently, the knowledge provided by the present invention is not predictable and it would not occur to one of ordinary skill in the art.

Thus, there is no suggestion or other basis for combining from Iida et al. that the entire plane is made to be N-region and from Fujikawa that the carbon is doped. Even if one of ordinary skill in the art were to attempt to combine such references, it cannot be directed that the pulling rate would be made faster.

On page 16 of the Examiner's Answer, the Examiner argues that the pulling rate can be determined by routine experimentation and usable wafers can be obtained from the description at lines 20-50 of column 14 and Figure 10 of Iida et al. According to the description referred to, two crystals are pulled in exactly the same manner, with one of the crystals being sliced and the other crystal being split longitudinally, to observe the distribution of the crystal defect. This is intended to demonstrate the effectiveness of the development described in Iida et al. at lines 51-54 of column 14. Such description does not at all suggest that a longitudinally splitting experiment is also performed in carbon-doping with no change of temperature gradient  $G$  and pulling rate being made to be faster. To begin with, it is not even suggested to combine carbon-doping and the N-region. Therefore, it cannot result from Iida et al. and Fujikawa that by combining carbon-doping and the N-region, a longitudinally splitting experiment is further performed, and the

pulling rate is further made to be faster. Accordingly, the present invention can be arrived at only in accordance with the teachings thereof and not from such prior art references.

Regarding the discussion at line 16 of page 16 through line 13 of page 18 of the Examiner's Answer, Iida et al. merely teaches a specific embodiment. A person of ordinary skill in the art would not expect the optimal pulling rate to be the same for all processes. The pulling rate taught by Iida et al. would not result in a crystal having an N-region over an entire plane of the crystal for a carbon doped crystal. Therefore, one of ordinary skill in the art would be required to perform routine experimentation to determine the value for the pulling rate required to form an N-region over the entire plane of the crystal. And, the Examiner maintains that a person of ordinary skill at the time of the invention when attempting to form an N-region in the carbon doped silicon single crystal taught by Fujikawa would have recognized that a faster pulling rate is required to achieve an N-region over the entire plane of the silicon single crystal.

Thus, the Examiner's argument is made with the benefit of hindsight afforded by the present invention. One of ordinary skill in the art would not expect the pulling rate to be the same for all processes, but it is common knowledge in the art that the distribution of the crystal defect becomes the same if the  $V/G$  value is the same. The temperature gradient  $G$  does not change by carbon-doping. It is common knowledge by those of ordinary skill in the art that the pulling rate  $V$  for the N-region does not change if  $G$  does not change. If this is different, then all of the descriptions in Iida et al., Fujikawa, Hourai et al., and so on, would be erroneous. More specifically, it was not known in each reference that the  $V/G$  value had a dopant-dependence. Accordingly, it is firstly recognized in accordance with the present invention that a crystal does not have an N-region over an entire plane of the crystal for a carbon doped crystal. Therefore, one of ordinary skill in the art would be required to perform routine experimentation to determine the value for the pulling rate required to form an N-region over an entire plane of the crystal, as the Examiner has argued. This is not described in the references, at all. First of

all, even though it is not described that a crystal is pulled by an N-region with carbon-doping, can it be recognized that the pulling rate is shifted. The references do not describe or suggest that one of ordinary skill in the art would not attempt to form an N-region in the carbon doped silicon single crystal. It would not at all be recognized that a faster pulling rate is required to achieve an N-region over the entire plane.

In lines 13-19 of page 18, the Examiner argues that the increase in the pulling rate for a carbon doped crystal is not significantly different from the value for an un-doped crystal. However, the difference in rates between 0.65mm/min and 0.54mm/min is more than 20%. Therefore, such difference is significant because the productivity of a crystal is drastically different. Moreover, the present invention has the effect that by doping the crystal with carbon, the formation of oxygen precipitation nuclei can be accelerated by heating treatment at a low temperature (see lines 16-23 of page 9, the last line of page 9 through line 4 of page 10, lines 3-8 of page 16, and so on). Therefore, the effects achieved in accordance with the present invention are clear.

Conventionally, it has been thought that the distribution of the crystal defect was determined only by the V/G value, as described in each of the references. The present invention is not obvious in view thereof. It is again respectfully submitted that claim 9 properly complies with the written description requirement, and that claims 1-10 are patentable over the various combination of prior art references.

This Reply Brief is submitted herewith in triplicate.



If there are any fees due in connection with the filing of this Response, please charge the fees to our Deposit Account No. 50-1314.

Respectfully submitted,

HOGAN & HARTSON L.L.P.

Date: May 17, 2005

By: 

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